REVIEWS.

ART. XIV.—*Clinical Lectures and Reports by the Medical and Surgical Staff of the London Hospital, with an Appendix on the Recent Epidemic of Cholera.* Vol. III. 8vo. pp. 499. London, 1866.

CLINICAL instruction has of late years been made a very prominent part of medical education, and very properly so; but its usefulness has been impaired often by abuses which have crept in, some of which are the necessary result of the very mode of education which is generally pursued in our profession. It will be profitable, therefore, as introductory to our notice of the book under review, to consider the proper place and function of clinical teaching in medical education.

It should not be entered upon at the outset of the course of study. There needs to be some degree of preparation on the part of the student to enable him to profit by it. The present custom is to have the fresh students attend with the advanced upon clinical lectures. The actual instruction imparted to the former is much less than that which comes within the grasp of the latter. It is so little, indeed, that it may be truly said that they had better occupy their time in some other way. But it is not a mere loss of time that is the chief objection to the plan. It is the injury that it inflicts upon the mental habits of the student. It tends to make him a poor observer and a loose thinker. He hears so much that he cannot understand, that he insensibly forms the habit of taking but little of what he hears fairly into his mind, and of holding that little very loosely. It may be in the case of a few who have uncommonly active minds, and who have been trained in strict habits of thought and observation, that what is actually understood is culled out carefully from what is not, and is garnered up in the memory. But there is no such sifting process in the minds of the great mass of students of medicine; and, therefore, the subjecting them to attendance on lectures which are for the most part beyond their comprehension, confuses their ideas, and materially impairs their mental vigor and clearness.

There is another point of importance. The profit which a student gets from instruction depends upon the interest which it awakens in his mind, and this again depends chiefly upon the degree to which he understands what he hears. If then the great majority of what he hears is beyond his comprehension, how much must his interest be dampened, and how injurious is it to have this effect produced upon him at the very outset of his education.

There is another point still. So shadowy and confused are the glimpses of truth which the fresh medical student gets in the clinical lecture, that some errors will inevitably be lodged in the mind, which may or may not be got rid of in future instructions. And errors are certainly most of all to be deprecated in the practical part of our teaching.

This misplacement of clinical instruction is only one phase of a general fault that exists very glaringly in the prevalent symptom of medical education. The fault is a radical one. The want of due sequence and gradation in studies is the grand cause of failure, and the looseness of thought and observation thus engendered stamps its character upon medical practice, and of course upon medical literature. We would like to dwell at large upon this point, but this is not the place for it.

A word on the style of clinical lecturing. It sometimes is not really clinical, but is like the general lecture, the only thing clinical about it being an occasional and perhaps slight reference to the patient. Like the text of some sermons, he is almost lost sight of, and a full lecture is given not only upon the malady from which he is suffering, but upon other kindred maladies also. The clinical lecture should be especially upon the points presented by the cases, particularly in regard to diagnosis, and the aims of medication; and full descriptions of the diseases should be deemed out of place. No one should ever have occasion to say of a clinical lecture on a patient, as we have sometimes heard it said, that for all practical purposes the patient might as well have been out of sight.

It should be made quite an object to exhibit to the students the modes of investigation which should be pursued in making out a diagnosis in different cases. We refer both to the direct examination of symptoms, and the sifting process, which more or less has to be applied to the statements of patients in regard to their own cases. The unravelling of a case is often the very highest instruction that can be given, and it is what cannot be imparted in a general lecture on disease.

No lecture has more of the essentials of interest than a clinical lecture. Whatever is practical is always interesting. Then, besides, there is the actual presence of the thing talked about. And there is added also, we think we may say very commonly, the interest of sympathy with the diseased and suffering. Now, there are some that seek to add to all this a sort of theatrical charm, degrading the clinical lecture to an occasion for self-display and the mere idle gratification of the audience. And sometimes, we regret to say, that in doing this delicacy and propriety are outraged.

Hospitals present fine opportunities for furnishing to the profession extensive reports of classes of cases. There is an advantage here over private practice in many respects—in the number of cases of any one disease, in the facilities for making minute records, and in the entire control of all the circumstances of the patients. At the same time it is to be remembered that hospital practice and private practice must necessarily be often different from each other. In acute diseases in hospital practice there are more often cases brought under treatment that are at an advanced stage, or that perhaps have been already treated, and in what way it is either known but partially or not at all; and in chronic diseases the patients have very commonly gone through a considerable experience of suffering, and of treatment, medical, or quackish, or both, before they come to the hospital. To say nothing of other differences, there is more call for stimulants and tonics in hospital than in private practice. Full reports of both, so that the requisite comparisons may be made, would be exceedingly valuable. We have not enough of such minute records. Conclusions, inferences, summings up of results we have in great abundance. But these take too much the colouring of the opinions of the individual, which very often may be found to be somewhat inconsistent with the facts, if they are recorded with any degree of faithfulness and minuteness. The great need of the profession in therapeutics is full records of cases, giving in each disease the mild and the severe alike, and also those in which the treatment is unsuccessful as well as those in which it is successful. On this latter point there is often a glaring defect. The account which Graves gives of the use of tartarized antimony and opium in certain conditions of typhus fever, valuable and significant as it is, is much marred in this respect. He reports quite circumstantially all the cases in which the treatment resulted in recovery, but while he refers to cases in which it was not successful, he does not give us the details of one of them. These might have thrown some light upon the proper limitations of the practice, and cleared up some points that need to be cleared up. We would not be understood as throwing any doubt upon the real value of the contribution that he has thus made to our therapeutics, for we have witnessed ourselves in the most marked manner the success of the practice; but the omission referred to has prevented him from giving to us its full value.

Reports of cases may be full, and yet may be very poor reports. A minute observer may not after all be a good one. He may make altogether too much of some facts and too little of others, and this fault will be seen in his description of cases. In the midst of his minuteness he may even overlook some circumstances that are of prime importance. In a good and correct account of a case all the facts are brought out in their due proportion. To do this, there is required, first, skill in observation, and second, skill in representation, the first being a *sine quâ non.*

Sometimes we find strange omissions on the part of those who may be called good observers. This is commonly owing to the influence of some opinions that bias the mind in its observation. One of the most striking examples of this omission we have in Louis' two volumes of cases of fever. Here, with extraordinary minuteness of observation on most points, the record of the number and the character of the alvine discharges is exceedingly meagre. And yet observation should certainly have been decidedly directed to that quarter, in connection with the lesion which was found so uniformly in the intestines.

It is minute, judicious, and comprehensive observation that is to do the great work in diagnosis and therapeutics. To this has our profession very much come at last, after ages of blundering about among dazzling theories. The time has gone by for the framer of an ingenious general theory of medicine to bear rule. It was because this reign of rigid observation was setting in that Broussais, the last of the long succession of theorizers, unlike his predecessors, lived to see his theory die out, though at the first he had multitudes of adherents. The question now is not so much what one thinks, but what does he know—what has he found out—what has he proved. Suppositions, indeed, on particular points are indulged, as pioneers in getting at the facts; but these are very different from those compounds of facts and suppositions which have been termed theories in medicine in the ages that are past.

In this work of observation our hospitals are to bear a prominent part, from their facilities for making it thorough, exact, and comprehensive. We are glad, therefore, to see that some of them are publishing annual reports of their observations and clinical lectures. The volume before us is a collection of this character for the year 1866.

With these preliminary observations we propose to notice some of the contents of the volume.

One of the most interesting papers in the volume is on Spectrum Analysis, by Dr. Letheby, the Professor of Chemistry at the London

Hospital College. What is said of the physiological and medical uses of this means of investigation is preceded by a historical account of the successive discoveries that have led to the rich facts which are at length developed. A brief summary of these will not be uninteresting to the readers of this Journal.

The discovery of the solar spectrum was made by Sir Isaac Newton, in 1701. His experiments were repeated for a whole century without adding anything in the way of discovery. But in 1802, Wollaston, by using a narrow slit instead of a circular aperture for the admission of the sun's rays, produced a far more perfect spectrum, and dark bands, not before seen, now intersected the colours. He also examined the spectra of different kinds of light, as the flame of a candle, and electric light, and he noticed that the spectra differed much from each other. And here was in reality the starting-point of spectrum analysis. But how much significance there was in these dark bands or lines, and in the differences of the various spectra, was utterly unknown to Wollaston, and was to be discovered only by multiform and long-continued investigations. Indeed, the researches of a half century have probably fallen very far short of developing all of it.

In 1815, Fraunhofer, a German optician, fixed the prism before the object-glass of a telescope, and the spectrum being thus magnified showed the dark lines seen by Wollaston so distinctly, that he recognized and marked the relative position of over six hundred of them, distinguishing the most important by capital letters, and the least important by small ones. He knew that these lines must mean something, and though he did not himself divine their meaning, he with true scientific zeal did the work, and placed the results on record—results which have since proved to be of great value, being constantly referred to in investigations at the present time.

In 1822, Sir David Brewster examined the spectra of flames coloured with various salts, such as are seen displayed in pyrotechnic chemistry. Sir John Herschel, in the same year, pursued the same line of investigation. He stated as the result, *that the colours communicated by the different bases to flame afford, in many cases, a ready and neat way of detecting extremely minute quantities of them, and that the tint arose from the molecules of the saline matter reduced to vapour, and held in a state of violent ignition.*

A year or two later, Mr. Fox Talbot pursued the investigation still farther, developing new facts which illustrated beautifully the truth above stated.

In 1835, Mr. Wheatstone gave an account of the spectra produced by the prismatic decomposition of the electric, voltaic, and electro-magnetic sparks, and also of the different spectra when the sparks were taken from different metals; and after many observations, he states that the appearances are so characteristic, that by this mode of examination the metals may be readily distinguished from each other. He also discovered that the results were not produced by combustion, for the appearances were the same when the sparks were taken in the vacuum of an air-pump and in carbonic acid gas.

From this time there were many observers working in this field, bringing out many valuable facts, and approaching very nearly to conclusions which, by patient and varied investigation, were reached many years afterwards. For example, Dr. W. A. Miller, of Cambridge, England, said, in 1845, as a result of his observations, "It would therefore appear that luminous atmospheres exist in which not only certain rays are wanting, but which exercise a positive absorptive influence on other light"—a very strong foreshadowing of an important discovery announced by Kirchhoff, in 1859, which we shall notice soon.

The delicacy of the test which spectral analysis offers is wonderful. Swan found, for example, that he could detect the presence of the $\frac{1}{2500000}$th of a grain of sodium by its characteristic yellow line; in fact, the reaction of this metal is so delicate, and the presence of common salt in air and water is so universal, that it is very difficult to get a flame free from the sodium line. Bunson and Kirchhoff deflagrated different substances in a room, noting the prismatic effects. Calculating from the area of the room and the quantity of matter diffused, they estimated that it was possible to recognize the $\frac{1}{60000}$th of a grain of potassa or baryta, the $\frac{1}{1000000}$th of a grain of lime or strontia, the $\frac{3}{60000000}$th of a grain of lithia, and the $\frac{1}{180000000}$th of a grain of soda. And the presence of these different substances can be discovered in the spectra even when they are mingled together, for as they are volatilized at different temperatures, the several spectra follow each other in the order of their volatility.

"Other examples," says the lecturer, "of the delicacy of the test may be given, to show how certainly and easily the minutest portion of these alkaline metals may be discovered. If a little of the ash of a cigar is put upon a platinum wire, moistened with muriatic acid, and then heated in the flame, it shows the characteristic lines of sodium, potassium, lithium, and calcium; and the residue of a single drop of sea-water, treated in the same manner, exhibits the spectra of sodium and calcium. A drop also of the mother-liquor of almost any mineral spring, will show the coloured lines of nearly all the alkalies and alkaline earths.

"It is, therefore, easy to understand how, by this method of research, traces of substances have been found, where they were not suspected to exist, and where, but for this process, they might have remained forever undiscovered. The alkali lithia, for example, once thought to be so rare, is now known to be widely distributed. It has been recognized in a large number of mineral waters, in the ashes of tobacco, in vine leaves, grapes, the tartar of wine, the potashes of commerce, and, indeed, in the ashes of all plants which grow upon certain granitic soils. It has also been found in the milk of animals feeding upon those pastures, in human blood, and in muscular tissue. Strontia also, instead of being a comparatively scarce substance, exists nearly everywhere in the mineral kingdom: it is present in sea-water, in the incrustations upon the boilers of sea-going vessels, in chalk, in certain marbles, and in a great variety of limestones. The new metal, thallium, has likewise been found in many pyritic minerals, associated with copper and iron; and it has been recognized in the ashes of the grape, tobacco, and chicory; and in treacle and wine. The extent to which these researches may be applied in gaining information of the mineral constituents of animal and vegetable tissues is almost unlimited; and the influence of them on the future of physiology is hardly to be conceived."

By this minute mode of analysis several new metals have been discovered. On this point Dr. Letheby says:—

"I may mention that while Bunsen was in this way examining the residue of a mineral spring at Durkheim, he observed the appearance of certain blue and red bands in the spectrum, which were not common to any known substance, and by pursuing the inquiry, he discovered two new metals of the alkaline series which he named *Cæsium* and *Rubidium*. The former was so named, because of its producing two characteristic blue lines of a sky colour (*Cæsius*), and the latter from *Rubidus*, dark red, in consequence of its showing two red lines at the very edge of the red end of the spectrum. The quantity of these metals in the Durkheim water was not more than about three grains per ton of water. In like manner *Thallium* was discovered by Mr. Crookes, in March, 1861, in the seleniferous deposit from the sulphuric acid manufactory at Tilkerode, in the

> Hartz Mountains: and it was so named from *Thallus*, a budding twig, on account of the beautiful green line which distinguishes its spectrum. *Indium* also, a fourth new metal, was discovered in 1863, by MM. Reich and Richter, of Freiberg. in a Saxony zinc-blende, and was thus named because of the two splendid indigo-blue lines which characterize its spectrum. The first of these metals, Thallium, is of the nature of lead, and gives a bright green tint to a colourless flame; while the second, Indium, is closely related to zinc, and gives a rich violet-blue colour to flame."

Of all the spectra those of the alkalies and alkaline earths are the most characteristic, and they are the most easily obtained. In regard to the metals, the proper way to develop their spectra is to convert them into volatile salts, and of all salts the chlorides are the best.

Mr Reynolds has contrived a simple but ingenious plan by which nearly all minerals can be made to show the spectra of their constituents. He uses the double blowpipe jet of Mr. Herapath, and by supplying it with hydrogen, and blowing steam charged with muriatic acid through the inner jet, he decomposes the fragment of mineral held in the flame, producing chlorides, which give their characteristic spectra. The spectra of the less volatile metals are best observed by letting the electric spark between fine wires of them. The peculiar tint of the metal is displayed in the spark by the volatilization of a minute quantity of it by the heat and the disruptive force of the electricity.

Non-metallic substances, as well as metallic, have been subjected to spectral analysis. In all cases the character of the spectrum is influenced by the degree of heat applied, becoming both more brilliant and more complex as the heat is increased. In fact, a solid or liquid, so long as the heat is not sufficient to make it otherwise, though it may produce luminosity, will give only a continuous spectrum, having simply the tints of the rainbow; and it is only when the gaseous flame or electric spark changes the substance into an incandescent, gaseous, or vaporous matter, that the characteristic non-continuous spectrum is produced.

We have already alluded to the discovery of Kirchhoff, foreshadowed by Dr. Miller. Others approached even nearer to it than he did, as Foucault and Augstrom. The discovery, which was not made till 1859, was this—that the rays which a substance emits are the very rays which it absorbs. For example, sodium, when ignited, emits an intensely brilliant yellow light, which makes in the spectrum two closely contiguous bands of yellow; but if through the flame of sodium, the more powerful beams of the electric or oxyhydrogen light are transmitted, black lines appear in place of the yellow sodium lines, showing absorption. Kirchhoff also ascertained that certain of the bright bands in the spectra of potassium, lithium, barium, and strontium, may, in like manner, be reversed.

On this Dr. Letheby remarks:—

> "The explanation of this is very simple, and it applies to the corresponding phenomena of radiant heat, as well as to those of actinic or chemical action. 'Atoms,' says Professor Tyndall, 'which swing at a certain rate, intercept waves which swing at the same rate. The atoms which vibrate red light, will stop red light; the atoms that vibrate yellow, will stop yellow; those that vibrate green, will stop green, and so on. Absorption is a transference of motion from the ether to the molecules immersed in it, and the absorption of any atom is exerted chiefly upon those which arrive in periods coinciding with its own rate of oscillation.'[1] This is true of every variety of vibration, and in the case before us the vapour of a metal put in the tract of a beam of light, will

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[1] Heat considered as a Mode of Motion, p. 468.

stop its own peculiar vibrations, and, therefore, will produce dark bands in those very places in the spectrum where its own characteristic coloured bands would have appeared. Or if the bands are obtained from the incandescent vapour of a metal, and the light thereof is made to pass through a similar vapour, the course of the light is stopped, and there are no bands at all. The reason, therefore, why incandescent solids and liquids exhibit a white light, and produce a continuous spectrum, consisting of all the prismatic colours, is that their atoms or molecules swing in every variety of vibration from the red to the violet; whereas the vapour of an element swings only in certain vibrations, and thus produces a coloured flame which gives only the few bands of the spectrum which are characteristic of that flame."

And now we come to some most wonderful results of these discoveries. In regard to them Dr. Letheby says:—

"The application of these facts to the interpretation of the luminous phenomena of the universe, and more especially to the analysis of celestial objects, is most interesting; for as the results are not influenced by distance, it follows that whenever there is light enough from any planetary body to produce a spectrum, it affords the means of ascertaining its nature, and of determining its condition. If, for example, it is found that the light emitted by any star or far-off luminous object gives a spectrum with the characteristic bands of ignited terrestrial matter, it is proof that such matter exists therein; and if the spectrum is continuous, it is manifest that the light comes from an incandescent solid or liquid; whereas if the spectrum is not continuous, but consists of only a few bands of colour, it shows that the light is emitted by matter in a state of gas or vapour. And if, in addition to the continuous spectrum, there are dark lines in it, it proves that there is a central mass of ignited solid or liquid matter, with an atmosphere or envelope of vapour through which the light passes, and by which certain rays are absorbed or darkened. This is the interpretation of the dark lines of Fraunhofer in the solar spectrum, which have been for so long a time the puzzle to philosophers. According to the hypothesis of Kirchhoff, the sun consists of a central orb of molten or solid matter intensely heated, and as this emits rays of all kinds of refrangibility, it would give a continuous spectrum, without a break or bar in it, were it not that it is surrounded by a vaporous envelope through which the rays must pass; and in so doing they are partly absorbed; for each element of the gaseous envelope strikes down the very rays which it does itself emit, and thus produces its own black lines. If we could see the spectrum of the envelope alone, without the luminous orb behind it, we should see the very bands, which are now dark, lighted up with their characteristic tints. It would be, in fact, the complex spectrum of all the elementary gases and vapours existing in it. Examined, therefore, from this point of view, we have in the black lines the dark or reversed spectrum of every element in the solar atmosphere; and we have but to compare them with the spectra of terrestrial vaporous matter to determine their nature. In this way the labours of Kirchhoff and Bunsen have been directed to the spectral analysis of the sun. The position of every line has been compared with the spectral lines of terrestrial matter; and although much has been done in determining the chief constituents of the solar atmosphere, yet as each line must be accurately compared in every particular, it will take many years before the whole of them can be properly studied. Already about 2000 of them have been thus mapped out; and they indicate the presence in the sun's atmosphere of sodium, calcium, barium, and magnesium; with iron, nickel, cobalt, chromium, copper, and zinc. There may also be potassium and strontium; but there are no traces of lithium, cæsium, rubidium, aluminium, arsenicum, mercury, silver, gold, cadmium, tin, lead, silicium, or antimony."

This celestial chemistry has been earnestly studied by many observers, Kirchhoff and Bunsen in Germany, Donati in Italy, Miller and Huggins in England, and Cooke in this country, and the results thus far are of the most interesting character. The moon is proved by the spectrum which it gives to have no atmosphere, while Venus, Mars, Jupiter, and Saturn show that they have an atmosphere, by certain dark lines upon their spectra. About fifty of the fixed stars have been examined, and as their spectra are very nearly the same as the solar spectrum, it is inferred that they are centres of light—self-luminous, and of the same structure as our sun. The nebulæ have been examined, and while the results obtained coincide with the observations made with the telescope, in eight instances where the telescope has not been able to discover distinct points, it has been concluded from their spectra that they will never be resolved into groups of stars, but are only masses of luminous gas.

In May last a new star was discovered in the constellation of the Northern Crown, which, examined by Dr. Miller and Mr. Huggins with the prism, revealed some singular phenomena. There were three spectra—one like that of the sun, another with dark lines, and over this a third, with very bright lines. It was concluded, therefore, that this star was like the sun and the fixed stars, an orb of incandescent matter, with an atmosphere of cooler vapours, and that it also had another atmosphere which was still more luminous. And as there had been a sudden outburst of brilliancy in the star, it is inferred that some great convulsion took place which evolved large quantities of gas, and that it is the burning of hydrogen in its combination with some other element, that produces the bright lines in the spectrum; also, that this combustion has heated to vivid incandescence the solid matter. Then, as the hydrogen becomes exhausted, all the phenomena become less intense, and the star rapidly wanes.

We pass over some very interesting statements in regard to the spectra of the rays of heat, and the actinic or chemical rays, and come to what most interests us as medical men.

This spectrum analysis is capable of very broad application. It has been extended into physiological researches, and used as supplementary to investigation by the microscope, confirming its results, or even settling questions where the microscope fails.

The notice of its use in these investigations is thus introduced by Dr. Letheby:—

> "That, however, which is especially interesting to us is the application of chromatic spectrum analysis to the recognition of organic and other colouring-matters, and to the discovery of their chemical and physiological nature. The blood, for example, when examined for its spectrum under the action of different reagents, shows that the colouring-matter is principally concerned in the phenomena of respiration: and the appearances of the spectra are so remarkable, that they become the means of discovering the presence of blood in very minute quantity. Professor Hoppe was the first to notice this;[1] and he became acquainted with it by passing a ray of light through a weak solution of blood, and examining it with a prism: he then observed that the blue end of the spectrum was cut off, and that two dark bands appeared in the green. The same effect was produced with the red blood of all animals, and he, therefore, proposed to use it as a means of medico-legal research. Later still, in 1864, Professor Stokes continued the inquiry;[2] and he ascertained that the colouring-matter of the blood, when fresh, was very different from the hæmatin of Lecanu; for it was soluble in water, and had the power of absorbing oxygen, and giving it out with great facility, and that the spectra in the two states of oxidation were very different. He, therefore, named the substance *cruorin*, distinguishing the bright red arterial or oxidized cruorin by the name of *scarlet cruorin*, and the purple or venous deoxidized cruorin by the name of *purple* cruorin."

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[1] Virchow's Archiv. vol. xxiii. p. 446 (1862).

[2] Proceedings of the Royal Society, vol. xiii. p. 355.

The important distinction between the hæmatin of Lecanu, and the cruorin of Stokes, appears very decidedly in the comparison of the spectra. Hæmatin is cruorin chemically changed, either quickly by the application of certain acids, the acetic, citric, or tartaric, or gradually by atmospheric action, probably of an acid character. Cruorin exists in two states, as *scarlet or oxidized cruorin, and purple or reduced cruorin*, and the spectra of the two are different from each other. The same is true also of hæmatin.

Long before these investigations of Stokes, grave doubts were entertained of the identity of hæmatin with the colouring matter of the blood, as it appears in the red corpuscles. Lehmann expressed himself very strongly on this point, saying that it was by no means certain whether it is a product of metamorphosis of the true colouring matter of the blood, or whether the hæmatin obtained bears the same sort of relation to that in the blood-corpuscles as coagulated albumen bears to that principle in its fluid state. But it now appears that it is a real metamorphosis, a chemical change that produces it from the cruorin of Stokes.

Cruorin very greedily absorbs oxygen, and also very readily parts with it under the action of reducing agents. The oxygen is, therefore, as it unites with it in respiration, not chemically combined with it, or at least has some kind of loose union with it, "something" as Dr. Letheby says, "like that which fixes gases in water, or which holds the second molecule of carbonic acid in bicarbonate of soda, all of which are easily displaced."

There are both scarlet and purple cruorin in venous blood, the latter being even in comparatively small proportion. This has an obvious relation to the fact that both oxygen and carbonic acid are contained alike in arterial and venous blood, the proportions being different in the two cases, the oxygen being to the carbonic acid as six to sixteen in arterial blood, but as four to sixteen in venous.

The delicacy of the spectrum analysis in the detection of the presence of blood is very remarkable. The $\frac{1}{100}$th of a grain of liquid blood may readily be made to exhibit all the characteristic spectra. As an example of the delicacy of the tests, Dr. Letheby states that in the year 1849 he had occasion to make a medico-legal investigation of some blood-stains on linen, and the specimens which have been kept to this time (seventeen years) have been recently examined both by Mr. Sorby and himself with the production of the characteristic spectra with perfect distinctness.

That spectrum analysis will hereafter be of decided service in pathological investigations we have much reason to expect. We close this notice of Dr. Letheby's valuable paper by quoting a paragraph on this point.

> "As to the change which the colouring-matter of blood undergoes, when it passes from cruorin to hæmatin, we have but little knowledge, except that it occurs spontaneously, and under the influence of very slight causes. How far this may be concerned in the development of pathological phenomena is hardly to be surmised. The different appearances of the blood, in certain diseases, not only when it flows fresh from the vein, but also when it is seen in the dead body, may, perhaps, be due to abnormal differences in the proportions of scarlet and purple cruorin, or even to the presence of their altered products—hæmatin. Already the observations of Engel and Rokitansky have discovered a more than possible connection between the colour and consistence of the blood in the dead body, and its tendency to soak or diffuse into the tissues, and accumulate in certain organs, and they have even classified these physical properties of the blood into six kinds—each characteristic of a special group of diseases. And if to these observations, which are purely pathological and anatomical, there are added the exact investigations of the chemist, and the careful results of a

searching spectrum analysis, we may hope for a rich accession of scientific facts."

In a paper on diabetes there are six cases reported, three of which ended fatally, and the other three were discharged at their own request, there being some improvement, but no evidence, however, of a real cure. The treatment was, as usual in this disease, very unsatisfactory, and nothing new appears in the article as to the pathology of the disease, though a summary is given of the opinions that have been broached in regard to it.

In a paper by Mr. Hutchinson, surgeon to the hospital, and also surgeon to a hospital for skin diseases, there are details of five cases of unilateral herpes, or herpes zoster, and a tabular account of twenty-seven cases. He starts an interesting inquiry in regard to the nature of the disease thus:—

> "*Is herpes zoster an exanthem or a neurosis?* If an exanthem, why, then, is it not symmetrical, not attended by constitutional disturbance, not liable to spread by contagion? If a neurosis, why should it not relapse, why should it have stages, and how can it protect the individual against a second attack? There is no other neurosis which can be mentioned (neuralgia, for instance), which is not very liable to relapse, or to return again after cure. My own suspicion is that it belongs to neither of these classes, but that it constitutes a new group by itself, and further, I feel convinced that whoever may succeed in unravelling the mystery which at present surrounds it, must, at the same time, make a discovery in physiology."

He then proceeds to state and comment upon the following propositions.

Herpes zoster may occur at almost any age, and if we except early infancy, is equally frequent at all periods of life.

The two sexes are equally liable to its attacks.

It is not possible to denote any special condition of general health which predisposes to its attacks.

It is not contagious.

As a general rule it does not occur twice in the same individual.

The eruptions (with the very rarest exceptions) are never symmetrical.

It occurs with equal frequency on the two sides.

It generally observes closely the recognized anatomical distribution of some nerve.

The nerve affected is normally a cutaneous one, but this is not invariably or exclusively the fact.

The disease runs a definite course.

In regard to the frequency with which it occurs on the one side or the other we have heard it often asserted that it almost always occurs on the right, and this is certainly true of all the cases that we have seen of the disease on the trunk of the body. On this point Mr. Hutchinson says:—

> "An interesting instance of the fallacy of individual opinions when formed without careful statistical inquiry, may be mentioned in the fact, that Reil says that herpes 'always occurs on the *left* side;' and Biett, that 'in nineteen cases out of twenty it is on the *right*.' The two asserters are almost equally in error.
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> "Sir Thomas Watson, in fifteen cases, found it on the right in ten, and the left in five. Rayer, in fifty-three cases, right, thirty-seven, left, sixteen. Mehlis, of twenty-five cases, right, nine, left, sixteen. The total of these observers gives ninety-three cases, and fifty-six on right side to thirty-seven on the left. In my own experience, I have in sixty-two cases, seen it on the right in thirty-two, and on the left in thirty. We may, therefore, conclude that it manifests no very appreciable preference for one side over the other."

We hardly subscribe to the opinion of Mr. Hutchinson, that "shingles is not a disease of the skin in any correct sense, and that the so-called dermatologists have no claim to it whatever." We allow that nervous influence is indeed more decided and obvious in this than in other cutaneous diseases, but it is not enough so to take it out of their company. Herpes zoster is a cutaneous disease, perhaps we may say an exanthem, but one of a very peculiar character. In some respects it stands alone by itself, just as hooping-cough does among the paroxysmal neurose diseases. We apprehend that nervous influence has more play in cutaneous diseases than it commonly has credit for. Why do boils appear in one part of the skin after another? Why do cutaneous diseases ordinarily tend to diffusion? Is there a virus, a *materies morbi*, that develops disease here and there, or is the diffusion not generally owing, for the most part at least, to nervous sympathy inducing one part of an organ to take on an action similar to that going on in another part of it? This nervous influence we often see manifested in the generation of disease in an organ sympathizing with the part originally affected. One of the strongest illustrations of this that we ever saw occurred in a case of cancer of the mamma. We had removed the tumour, which was a very large one, and the patient some weeks afterward died of disease in the lungs. On examination after death we found cancerous deposit in the lung on the side from which the tumour was removed. There was a deposit in the skin along the cicatrix, which was shown also by the microscope to be cancerous, while there was none at all in the original seat of the disease. Now why was the deposit made in the lung? Perhaps it will be said that here there certainly was a *materies morbi*, a taint in the blood. Granting this, the question returns, why did it develop itself in that particular locality? There is no vascular connection to account for it. It must be owing to that sympathy between the lung and the exterior of the chest, of which we make a practical use every time that we apply a blister to the chest in the neighbourhood of an inflamed lung.

There are two clinical lectures on four cases of complicated stricture by Mr. Maunder. The details of the cases are given so clearly, and with such comments, that much valuable information is communicated in relation to the minute management of such cases. One of his practical deductions from one of the cases is this: That a bladder may be greatly distended, and yet a fair quantity may be passed daily without emptying that viscus. We believe that mistake is not very infrequently made on this point. Some little time since we attended an autopsy in which an enormous quantity of fluid (some quarts even) was found distending the bladder, and yet the patient was reported daily to the attending physician as having passed sufficient water. We are reminded also of another case, in which a young physician supposed certain apparently grave symptoms to indicate the onset of peritoneal inflammation. An older physician, who was called in consultation, suggested that the bladder might be distended, and that this might be the cause of the symptoms. But it was said that the nurse reported the water to be free. The catheter, nevertheless, was used, and the removal of a large quantity of urine relieved the patient.

There is a lecture also by Mr. Maunder on strangulated hernia, which is marked by the same practical good sense as are those on stricture. He says of the use of opium: "Speaking generally, I may say that this drug is a dangerous remedy *before* reduction, either with or without operation; but a valuable agent *after* reduction. There are exceptions to both statements." He says of its use before :—

"The drug checks vomiting, alleviates pain, and has a grateful influence upon the patient generally. But under these apparently happy effects, a mor-

> bid process is steadily at work, and the necessary operation is perhaps only performed when the pathological condition of the hernia renders it no longer capable of performing its function. An untimely dose of opium, besides blinding the unwary medical practitioner to the actual condition of his patient, may have a very unfavourable influence upon the mind of the sufferer himself. To illustrate this statement, I will call to your recollection a case of strangulated hernia, in the person of J. W—b, a patient, under my care, in Devonshire Ward, about three months ago. This man had been the subject of hernia for some years, and usually kept it reduced, but once or twice previously it had redescended for a few hours, but had occasioned no serious consequences. This time, however, the symptoms had been sufficiently urgent, and before I saw him, a dose of opium had been administered with the effect of relieving all pain, and of checking that functional derangement of the stomach (vomiting) upon which we place much reliance as an evidence of strangulation. He was so comfortable that he at first refused my aid altogether, and at length gave an unwilling consent that if the taxis under chloroform failed, I might use the knife. The operation disclosed a very tight stricture at the neck of the sac. The man recovered."

Mr. Maunder never uses opium before reduction, unless he has previously determined to effect reduction by operation in the course of a certain space of time, when the taxis, assisted by chloroform or warm bath, fails to do so. In regard to its usefulness after reduction, he thinks that it is almost always of great service in securing repose to the damaged bowel, but that it may do harm when feebleness in the patient disposes to accumulation of gases in the intestines, by disabling the muscular coat in its resistance to the distension; in that case he would omit the opium and give brandy; but we doubt whether the opium should be so entirely renounced, for with the feebleness there may be irritation that needs to be subdued with the tympanitis.

In an article by Dr. Little, containing "Notes of unsuccessful and successful cases of saline alcoholic injections into the veins for relief of collapse of malignant cholera," the notes of eight cases are given exactly as they were taken down at the time, and their fulness makes them valuable, because, with other similar accounts which we hope will be gathered of observations that have already been made, and of others to be made hereafter, they will enable us to come to a right conclusion in regard to the value of venous injections. From the temporary effects produced in cases that ended after all in death, and the manifest bearing of these effects on the final result in the successful cases, we cannot help hoping, with Dr. Little, that further trials—increasing, of course, knowledge and skill—will enable us to pronounce the measure to be very efficient in recovering patients thoroughly from the state of collapse. Dr. Little says, in relation to past trials:—

> "When we take into consideration the difficulty of determining with any approach to accuracy, the proper density of the fluid to be introduced into the altered blood of cholera patients, without risk of the injury to blood-corpuscles, and possibly to other elements of the blood; the paucity of reliable analyses of the blood of cholera patients; the difficulty in making arrangements in private houses and temporary cholera hospitals, for due preparation of materials; the imperfection of the injecting apparatus employed, we may justly experience surprise at the number of recoveries which rewarded some operators, rather than at the number of deaths which sometimes succeeded the operation."

It may be, we would add, that the application of the remedy has often been made later than it should have been, as is often true of other extraordinary remedies in other diseases, and that this is the cause of its alleged want of success. In Dr. Little's cases three of the eight were cured, and in three of the five cases that ended in death, the patients were pulseless when the injection was used.

It is of importance that the apparatus be properly arranged, and also properly managed. Dr. Little's apparatus is simple, and yet complete and excellent. A representation and full description of it are given. The composition of the fluid which he injected was as follows: Sodæ hydrochloratis ʒiij; sodæ sesquicarb. ℈ij; aquæ destill. ℥cxx, carefully filtered, and subsequently heated. To each imperial pint (℥xx) immediately before injection, at temperature of 110° to 115° F., two drachms of alcohol were intimately mixed by stirring.

Two gold medals are annually awarded to students for general proficiency in medicine and surgery. The award is made not alone upon an estimate of the relative quality and amount of work done by the candidates in the wards as dressers, &c., but they are obliged to undergo an examination on clinical subjects, and furnish notes of at least six cases. The notes of twenty-two of these cases are given. We notice, in passing, one fault in some of them—the failure to state the amounts of some of the remedies administered. In all accounts of cases we think this point should be quite scrupulously attended to, but it is often neglected even by prominent men in the profession.

Among these reports we find three cases of dislocation of the femur on the dorsum of the ilium. The plan of reduction which it is said is "almost always adopted with success at this hospital," is thus described: "The pelvis was steadied; the leg flexed on the thigh; the thigh adducted and flexed on the abdomen; abduction was then commenced, and during this the head of the bone slipped into the socket." There is a remarkable resemblance in this to a description given of a reduction of the same dislocation, effected probably more than fifty years ago by Professor Nathan Smith, of New Haven. It is thus stated by his son, Professor N. R. Smith, of Baltimore: "After attempting the ordinary methods by extension in vain, he bent the leg upon the knee, seized the leg, and, using it as a lever, rotated the thigh a little outward. Then he gently abducted the thigh, and lastly flexed it freely on the pelvis, by carrying the knee toward the face of the patient. These movements instantly succeeded, and with but little effort of strength."

There is a short paper by Mr. Maunder, containing a description, with a plate, of a lithotrity syringe, which we commend to the notice of surgeons.

An account is given of a case in which gastrotomy was performed by Mr. Curling, for stricture of the œsophagus. Here was attempted to be done by design what was done accidentally in the famous case of Alexis St. Martin. But the circumstances of the two patients as to general condition were entirely different. St. Martin was in good health at the time of the accident; but the patient of Mr. Curling had suffered from the disease in the œsophagus for a long time, and had not only become much exhausted and emaciated for the want of nourishment, but his digestive organs had become thoroughly deranged. Then the operation was delayed till the exhaustion was so extreme that the skin was cold, and the pulse was exceedingly feeble and intermittent. He died in thirty-two hours after the operation, not from peritoneal inflammation, but from sheer exhaustion, as should have been expected as a matter of course. It was of no use to perform such an operation on a patient so utterly exhausted.

The same error is often committed in other diseases—as, for example, croup. We would not discourage resorting to an operation if there be the least ground of hope from it, where other means have failed; but we are decidedly opposed to such a measure when it does not offer even the shadow of a hope. It is not enough to say that the patient will surely die if it is not done. If he will surely die whether it be done or not, the surgeon has no right to do it. Some, perhaps, will think that there is no occasion for these remarks. We think that there is. There is a looseness in the ideas of some on the point in question. We have known operations to be performed where there was not the least ground for expecting relief from them. In many of the cases where a surgical operation is resorted to because the medical means have failed, it should have been resorted to before; and the reason that it was not done in time is commonly this: When the physician fairly comes to the conclusion that there is nothing more to be hoped from medical means, there is apt to be delay from various causes—as effecting due consultation, spreading the matter before the friends and before the patient, if he be an adult, &c.—and all this time the chances for affording relief by an operation are diminishing. The time to do it is when the conclusion above referred to is arrived at; and when the necessity for an operation is probable, arrangements should be made beforehand, so that if the necessity comes there may be no delay.

A very suggestive paper on "Marriages of consanguinity in relation to degeneration of race," is by Dr. J. Langdon H. Down, assistant physician to the hospital. It is a very prevalent opinion that such marriages are uniformly productive of evil to a greater or less extent. Dr. Down does not believe this to be true. His observations are quite extensive, his notes referring to 1138 cases of idiots. A strong point which he makes is, that there are other causes which are largely to be taken into the account. To illustrate this he gives some particulars of twenty cases of idiocy taken at random. His general conclusion is thus stated:—

> "Consanguinity has doubtless the power of aggravating any morbid tendency, as I believe it has, of perfecting any good quality. Any statistics on the results of the marriage of relations are of doubtful value, unless they give the life-history of the progenitors. What a different aspect the whole matter assumes when this plan is adopted, will be apparent to the readers of this paper. Whenever a similar investigation is made, I believe it will be found, as in the subjects of my own inquiry, that consanguinity is only *one* of the factors, and not the most important one, in the production of deterioration.
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> "If our advice be sought, it will be our duty to inquire into other elements which are less on the surface, but which have equal or even greater potentiality for evil.
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> "Alliances, such as I have exhibited, with hereditary disease on both sides, should be discountenanced even where there is no element of consanguinity. It would only be a part of a true philosophy to render more forcible our opposition where blood-relationship would have a well-determined tendency to aggravate the wrong."

A similar opinion, derived from observations in the village of Batz, where marriage among near relations is of very frequent occurrence, has been given by Dr. Auguste Voisin, physician to the Bicêtre Hospital, Paris, a notice of whose book the reader will find in the January number of this Journal, p. 232.

Dr. Down also contributes a short paper on the ethnic classification of idiots. He does not go into the subject fully, but suggests enough to prompt farther investigation in that direction. He says that "a considerable portion can be fairly referred to one of the great divisions of the human family other than the class from which they sprung." The Mongolian type he finds in more than ten per cent. of the cases. If he is strictly correct in his observations there is some truth in the following remarks with which he closed his paper:—

> "Apart from the practical bearing of this attempt at an ethnic classification, considerable philosophical interest attaches to it. The tendency in the present day is to reject the opinion that the various races are merely varieties of the human family having a common origin, and to insist that climatic, or other influences, are insufficient to account for the different types of man. Here, however, we have examples of retrogression, or at all events, of departure from one type and the assumption of the characteristics of another. If these great racial divisions are fixed and definite, how comes it that disease is able to break down the barrier, and to simulate so closely the features of the members of another division? I cannot but think that the observations which I have recorded, are indications that the differences in the races are not specific but variable.
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> "These examples of the result of degeneracy among mankind, appear to me to furnish some arguments in favour of the unity of the human species."

In a lecture on cases of cerebral hemorrhage, by Dr. J. Hughlings Jackson, there are scattered through it some very sensible practical remarks on the values of different symptoms; the relations of this difficulty to disease in other quarters of the system; and the importance of recognizing fully the general condition of the patient. On this last point he thus remarks in relation to a patient, W. K.,[1] who is the chief subject of his lecture:—

> "The patient I shall show you, at the close of this lecture, has, no doubt, for years been getting into a condition in which the vessels of his brain would readily give way. Frequently, however, our patients know nothing, or they think little, of the serious changes which render them liable to such accidents. Their tissues become so universally, and yet so slowly, lower in vitality, that there are no particular failures to obtrude themselves until something gives way, or until some part becomes inflamed. And when a patient comes to us for 'winter cough,' I fancy even we may think too much of his temporary bronchitis, as he is sure to do, and too little of his permanent emphysema. If he consult us for pneumonia or pleuritis, we must not think we have got to the bottom of the matter by saying he has taken cold. A patient's pleura is not a thing apart from him, but a piece of his body; and when we hear a friction murmur, we must not treat *it*—if I may say anything so grotesque—but the inflamed pleura, and the man of whom it is a part. It, doubtless, simplifies matters, in a superficial sense of the word simple, to know that there is lymph effused, and to think of mercury, and to try to get the lymph taken up. Just so, it is very loose talk, talking of treating hemiplegia, for hemiplegia is not a constant quantity. Yet, the man who thinks of hemiplegia as an entity, and gives what he has found 'good for hemiplegia,' will have a certain kind of satisfaction. But it makes one less confident in the treatment of diseases of the nervous system to define severely when we can, the internal changes on

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[1] We have, in the account of this patient, a little insight into the drinking habits of the English. Thus Dr. Jackson says: "W. K. had, he said, been 'amongst beer all his life.' Yet, for a brewer's servant, he had been temperate. He had drank only five pints of beer a day, and 'a chance drop of spirits now and then.' It may seem odd to call this great excess 'temperance;' but I speak comparatively. I have now under my care for epilepsy a young man, also a brewer's servant, who says he has been in the habit of drinking twelve pints of beer daily. A gallon a day is not a very uncommon allowance. He got his beer for nothing, and it rarely occurs to patients in this rank of life, or in one a little higher, that now and then fluids, which cost much, had better be poured down a drain than down their own throats."

which the outward symptoms depend. When we can, as it were, see a great brutal clot, or a large, irregular, yellow-stained cavity, in the motor tract, we are less hopeful."

Again he says:—

> "It is wonderful how long bad materials will sometimes keep together. It may be, that although the patients are unsound, their general unsoundness is conservative. There is no healthy organ left to overwork the unhealthy ones. These patients descend into a lower sphere of vitality altogether—one which will do for routine work, for fair weather and for easy circumstances. The healthy human body, as Mr. Paget says, is fitted for the storms as well as for the calm of life. But patients like W. K. have no spare vitality, and, as I have said, are ready to give way under unusually unfavourable circumstances. They are ready to die at any point. A man with chronic Bright's disease may die of apoplexy, of bronchitis, of uræmia, of pericarditis, &c. &c., but to look on the pericarditis, apoplexy, &c.. in such cases as idiopathic, as essentially different things, rather than as different events, depending on one general condition, would be to think, as those historians seem to think, who write history from the reigns of kings, from battles and other striking events. It would be as reasonable to consider the outbreak of the French revolution as idiopathic, as it would be to consider W. K.'s attack of cerebral hemorrhage as idiopathic."

These remarks are very timely. Since the introduction, to so large an extent, into pathological investigation of the means of discovering what is local in disease, there has often been an undue tendency to look at the local to the disregard of the general condition. It is agreeable to both physician and patient to find something definite. Not only is it satisfactory, but it saves thought thus to narrow observation. It is at the cost of mental labour that all the intricacies of the complications of disease are ferreted out, and the condition of the patient, as a whole, is arrived at. And farther than this, great damage is often done to the patient by the undue localizing of disease. For example, we have known many a patient to have a prolonged sickness with her mind fixed upon her diseased womb, when we have no doubt that she would have had a comparatively quick recovery if she could have had all idea of that organ blotted out of her brain. We would not be understood as decrying the use of the speculum, but only its abuse. While there are many cases in which undoubtedly the grand difficulty is a local one, situated in the uterus, there are many others where, with other local or general derangements, there may be some slight disease of this organ, wholly subordinate, and easily removed with very little direct treatment, or with even none at all, but indirectly by the removal of other difficulties. It is in these last cases that the error we refer to is committed. It would be interesting to illustrate the same point by reference to diseases in other quarters of the system—as the lungs and the heart—but this is not necessary. The illustration which we have given of the prevalent tendency to make too much of local disease, is at the present time, perhaps, more often met with than any other; and we may add, that it is found often connected with quackery, and with what is quite as bad, if not worse, semi-quackery.

In a paper by Dr. Fraser, on Delirium Tremens with a record of eight cases we find nothing new. We believe with him that "without the action of alcohol there can be no delirium tremens." Other forms of delirium may resemble this in some particulars, but there are always some clear points of diagnosis. No one ever ought to make a mistake here. We do not believe, as many teach, that this disease uniformly arises from an abstraction of the usual stimulus. As it is apt to occur in an exhausted condition, as, for example, when the drunkard has been debilitated by sickness of any kind, so the exhaustion occasioned by the withdrawal of his stimulus may produce it; but it is by no means certain to do it, as the experience of jails and workhouses shows, and further than this, the disease often comes on when there has been no letting down of the usual daily amount of stimulus. We do not at all agree then to this statement of Dr. Fraser.

> "The point is whether the proximate cause is an excess of alcohol circulating in the blood, and acting lethally in the nerve centres; or from the deprivation of an accustomed stimulus, by which the tone of the nerve centres is morbidly lowered. This question is not one of theory merely; but also of treatment, as to whether stimulants are to be given, or withheld; for it is clear, that if an excess of alcohol is in the blood, instead of adding, we ought to eliminate; and, on the other hand, if the symptoms arise from a diminution of power in the nerve centres, owing to the absence of an accustomed stimulus, then alcohol will be the remedy."

This disease is not the result at all of alcohol now present in the circulation "acting lethally in the nerve centres;" neither does it arise simply "from a diminution of power in the nerve centres, owing to the absence of an accustomed stimulus." But it depends upon a peculiar condition of the nervous system induced by the continued irritation of alcohol. There may be nervous depression connected with it—there certainly often is—but we cannot say that it is essential to it. And as a matter of fact, some cases require stimulants, while others most generally do not. There is no one cure for the disease, and the most successful treatment is not that which follows any one mode, but which, proceeding upon general principles, accommodates itself to the varying conditions and complications that the disease presents. We distrust, therefore, all statements in praise of any one remedy or mode. We do not believe in large doses of digitalis, just now becoming quite fashionable. It may, for aught that we know, do as well as other exclusive modes, though we doubt it. In our experience, including the practice of other physicians as well as our own, delirium tremens is very apt to be recovered from, if it have no grave complications. We are not inclined, therefore, to resort to extraordinary means unless it can be distinctly proved that they are more successful than ordinary ones. Thus far we think that the most that has been proved in regard to digitalis is, that patients sick with delirium tremens sometimes are able to bear very large doses of it. But is it perfectly clear that some are not injured by it, as some have been by the large doses of opium, which, after the recommendation of Sutton, were some time ago administered so commonly by the profession at large? Digitalis was given in two of Dr. Fraser's cases—in one in which recovery occurred, but to what the recovery is to be attributed, Dr. Fraser is doubtful, because laudanum and chloroform were used as well as digitalis—and in the other, in which death resulted, the only medicine given being digitalis in large doses, and as the right side of the heart was found much dilated and the walls thinned, Dr. Fraser, inquires whether the effect of the digitalis was "too depressing upon an already weakened and dilated heart," in opposition to the doctrine that has lately been taught in regard to its action, which after all he thinks some facts favour.

There is a statistical report by Mr. Hutchinson, the surgeon to the hospital, of the deaths that occurred among his patients during the year 1865. The number of patients was 1,212, of which 81 died, a ratio of 6⅔ per cent. After a general table, there are particular tables of classes with quite full items. There is also a complete table of the cases of strangulated hernia for the year. There are also full classified statistics of all the major operations performed during the same year.

In a contribution by Mr. Hutchinson, on the results which follow injuries to nerve-trunks, there are full details given of seven cases. The observations were made some weeks after the injuries were inflicted, when in most of the cases the wound was healed. One very marked result was a remarkable loss of heat in the part which had lost sensation. The difference between the paralyzed and healthy parts, he found generally to be from six to ten degrees. It varied much with circumstances, because the temperature of paralyzed parts is very much at the mercy of external influences, the cold affecting them decidedly, while at the same time no applied warmth can raise them to the same temperature that it produces in the corresponding healthy parts. On the cause of this loss of heat Mr. Hutchinson thus remarks:—

> "To what are we to attribute this remarkable loss of power to sustain the normal temperature, and to guard against the depressing effects of external cold? To say that it is the result of mere disuse is unsatisfactory, for we find it just as marked where one finger is contrasted with the others on the same hand, as when the whole hand is affected. One of my cases (Case I.) is almost crucial in this respect, for the man had but a finger and thumb, and used neither of them, yet the finger deprived of nerve influence registered ten degrees lower temperature than the thumb which yet enjoyed it. To allege that it is due to 'dilatation of the smaller arteries and consequent slowness of the circulation' is scarcely more satisfactory, since paralysis of the vaso-motor nerve, and consequent dilatation of the vessels, is well known to lead *to increase of temperature*. Yet it is quite certain that the capillary circulation is in some way greatly disturbed. If you chill the part it becomes not merely pale, but livid; if you warm it, it becomes not of a bright pink, flesh tint, but of a peculiar dull brick-red colour. The most plausible conjecture seems to be, not that the nerve-control over the vessels is at fault, but that the *vis a fronte* is itself much diminished."

He supposes, and we think with good reason, that there occurs a change in the endowments of the cells of the paralyzed part from the lack of nervous connection, and that there is in them less disposition to growth and nutrition, making the circulation to be less active, and so occasioning a loss of animal heat. This condition of things seems not only to permit inflammation to occur, but to predispose to it, so that circumstances readily produce it. A peculiar kind of whitlow appearing on the tips of the fingers, was quite common in cases of section of the nerves of the forearm, in one case coming simultaneously on four fingers, which of course could not be owing to any accidental cause, as some pressure. It is reasonable, therefore, also to conclude that the inflammation of the eye, so apt to occur in paralysis of the fifth nerve, the formation of bed-sores and the occurrence of cystitis in paraplegia, are not altogether owing to accidental influences, but come in part from the lesion of the nerves.

Following this is a very instructive article entitled "clinical and pathological facts in reference to injuries to the spinal column and its contents." From the cases of the past two or three years, a selection has been made of those which illustrate important points in diagnosis, treatment, or physiological influence, nineteen cases in all. "The cases of recovery have especially been collected, with the view of illustrating the important fact, that even in severe cases, with permanent local displacement, and with paraplegia lasting some weeks, complete recovery may ensue." The cases of recovery are ten in number, in some of them it being incomplete. "Many of the cases give important information as to the kind of displacement which is most common, and its usual effects upon the cord, and illustrate clearly the inapplicability of operative interference. In no single case," it is added, "has any operation (other than attempts at reduction) been performed at this hospital."

In a lecture by Mr. Hutchinson, on the same subject, he urges very strongly this abstinence from operative interference. "My chief reason," he says, "is that, in operating, you convert a simple into a compound fracture, and add the risks of pyæmia, together with those of spinal meningitis. Then, I urge that depressions of bone very rarely exist, perhaps not once in twenty cases, and that it is utterly impossible to select the case." Again, he says, "My assertion is, that a good many cases recover, if put under favourable circumstances and let alone, and that of those which end in death very rarely, indeed, can it be asserted after *post-mortem* examination that an operation could by the barest possibility have done any good. In the great majority, then, since it could have done no good, its effect would have been to increase the patient's sufferings, and aggravate his danger." In regard to the infrequency of depression, he supposes that when the accident is so severe as to crush the spinal cord, generally the bones have sprung back by their own elasticity and that of the intervertebral substance, or that the displacements have been remedied by the mere straightening of the neck and trunk, which common sense dictates as the first thing to be done, and that where neither or both of these suffice, the replacement is commonly impossible. He says, also, that although much has been said about large effusions of blood into the vertebral canal as a cause of paralysis, such effusions are, he believes, the rarest of occurrences, for he has "never seen any effusion to the extent of possible compression, and in the majority of cases there is little or none." On this point, he remarks:—

> "The injury is a crush, and is not one at all likely to cause much bleeding. There are no large arteries to be injured. When you examine a specimen of fractured spine, you will find the muscles and soft parts externally much ecchymosed, there will be also a few small blood-clots adhering to the edges of the broken bones, but there will be little or no blood between the bone and the theca, and none at all in the thecal cavity. I speak of what is usual, and am well aware that exceptions may occur. The theca (dura mater) is very rarely torn, and often, on exposing it, you might imagine that the cord was not injured. Nay, even further than this, you find the pia mater of the cord entire and without ecchymosis, and only on slicing the cord through do you discover that its substance has been utterly smashed. I show you a sketch of a cord thus injured. You will see that the cord-substance, for nearly an inch and a half, is reddened by effused blood, and its substance broken into a diffluent pulp. Yet, in this instance, the pia mater was still entire."

There are many interesting points brought out in relation to the effects produced by injuries of the cord which we would like to notice if our limits would permit. We cannot forbear to quote a part of what is said of loss of sensation:—

> "If paraplegia be imperfect, you will always find that the patient retains more sensation than motor power. Sometimes he will appear to have lost motion utterly, and to retain sensation almost perfectly. This difficulty, in extinguishing sensation, is observed, I believe, in all diseases and injuries, whether of the brain or the cord. To a considerable extent it is, perhaps, apparent rather than real. Sensation is merely passive, motion is active, and the latter probably requires a far more efficient condition of the nerve-apparatus than the former. With a partially crushed spinal cord we may easily suppose a patient unable to transmit the orders of his will through the damaged portion, whilst still a certain degree of passive sensation (requiring no exertion of his will) may remain.

Another source of fallacy is the difficulty of accurate observations. A man tells you, 'I cannot move my legs,' and you are unable to prove the contrary, though it is still possible that a very vigorous exertion of the will might be able to set certain muscles in action; in other words, that voluntary motion, although seemingly in abeyance, is not absolutely lost. The same patient tells you that he 'can feel well,' yet very probably, if you try accurate tests, such as the compasses,[1] or drawing a hair or a feather over the surface, you will find that his sensory function is very far from perfect. On account of our frequent neglect of such tests we are compelled to receive with much qualification, recorded statements as to 'perfect sensation' being retained after these accidents."

We quote also what Mr. Hutchinson says of the use of the catheter, as worthy of consideration, though we think that, at the least, he makes too much of the influence of it in producing cystitis. The tendency of the injury is, as he states, to produce the disease, and the question is, whether this tendency is more decided with the use of the catheter, or with the distension of the bladder consequent upon not using it—a question which can be settled only by a careful comparison of many cases, such as remains to be made by Mr. Hutchinson and others, who have large opportunities of observation in this class of accidents. The passage is as follows:—

"There is, I think, room for much doubt as to whether the usual practice of relieving the bladder by the catheter is judicious. In a few cases where the fracture is in a certain part of the lumbar region, the bladder is involved in hyperæsthesia, and the pain caused by its distension, necessitates interference. These, however, are very rare, and, in almost all cases, the bladder fills without causing any discomfort whatever, and when full runs over. After a few days it regains a certain amount of tone, and empties itself very frequently. At this stage we have troublesome incontinence, and but little retention. Now, if the catheter be used from the first, inflammation of the urethra and bladder is, I think, almost certain to occur, and the urine will become loaded with pus and mucus. I suspect that cystitis is, in some cases, one of the influences which brings about the patient's death by exhaustion. Not unfrequently ulcerations of the mucous membrane of the bladder occur. There is a specimen in the Museum of a bladder and rectum from a fractured spine case, in which a fistula passes from the membranous urethra into the rectum, no doubt, in connection with the use of catheters. Why should cystitis thus constantly follow the use of catheters? Seeing that there is no impediment to the introduction of instruments, that they give the patient no pain, and are used with the greatest ease, why should they produce so much more irritation than we usually observe when they are employed for other reasons? I think we must admit, that it is probable that the mucous membrane of the bladder, when paralyzed, is in a state specially prone to inflame, just as the eye is, after paralysis of the fifth nerve. The practical question before us is, whether to permit the retention to continue until overflow takes place, is less likely to cause this cystitis than is the use of instruments. My own experience has been in favour of non-interference, and I quite intend, in the future, to make a full trial of this plan."

There is an appendix to the volume made up of notes on the cholera epidemic of 1866. As soon as it broke out, arrangements were made by the staff of the hospital for 38 beds in one of its wards, but this accommodation proving inadequate after the lapse of a week, two other wards were added, containing each 50 beds, making in all 138 beds for the exclusive reception of cholera patients. The largest number, however, at any one time in the hospital was 114. The whole number treated from July 10th to August 30th was 509, the deaths being 281, or 54.9 per cent. No cases of diarrhœa, however severe, were included in this statement. The mortality decreased week by week, it being the first week 85 per cent., and the last week 37 per cent. The gradual diminution of the mortality is accounted for by the difference in the severity of the cases. At the outset of the epidemic, most of the patients were brought in in a state of extreme collapse. "This epidemic," says Dr. Fraser, "began suddenly, raged fiercely, culminated sharply, and declined rapidly, as cholera has hitherto generally done, fitly represented by an ascending angle of 45°, and descending by one of 95°."

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[1] The æsthesiometer, devised, I believe, by Dr. Sieveking.

We will not enter into the discussion of the question whether cholera is contagious in view of the facts stated in the reports contained in this appendix, for such discussion, properly carried out, should take in the consideration of a wide range of facts accurately gathered. No narrow view of even well-observed facts, much less those which have been carelessly observed, will settle the question. We will give but a single quotation in relation to this subject:—

> "Some facts may be stated bearing on the question of contagion. At the height of the epidemic, upwards of one hundred persons were employed in the cholera wards; seven medical officers, five volunteer nurses, five sisters, eighty nurses, and five porters. None of the medical officers, volunteer nurses, or sisters, were attacked, and the porters enjoyed a like immunity. Of the nurses five contracted the disease, and of these four died. Three of those who died slept outside the hospital. In the laundry eleven women were employed in washing the linen, etc., from the cholera wards, and of this number, one was attacked and died. She was also a non-resident. As the mortality among laundresses is usually very large in a cholera epidemic, it would seem that the preventive measures adopted were not altogether ineffectual. No case of cholera occurred in the general wards of the hospital, although during the whole period of the epidemic, they contained their full number of cases, most of whom came from cholera localities."

The preventive measures referred to are those usually employed in the various modes of disinfecting, and these were employed very thoroughly.

Something is said of many of the remedies used in the treatment of cholera. Calomel is very decidedly recommended as having been found efficacious. Baths, at a temperature of from 98° to 104° F. were used with most marked effect in about 130 of the worst cases under the care of one of the physicians. The effects are thus described:—

> "Cramps ceased, anxiety of mind vanished, pulse returned, or, if originally to be felt, increased in volume and frequency. Many who had before moaned or shouted incessantly with pain, began to converse upon indifferent subjects, or in many cases sank into a tranquil slumber. Often recovery appeared to be the direct consequence of the bath, the improvement being permanent; but in many more, removal from the bath became the signal for the return, more or less rapidly, of the former symptoms."

Dr. Little, whose report of the effect of saline and alcoholic injections into the veins in the epidemic of 1849 we have noticed in another part of this article, followed up this practice in the present epidemic. He used now another apparatus, of which he gives a figure, one advantage of which is, that the operation can be readily performed with it by a single person. He reports fifteen cases. We find nothing new in the results as compared with those realized in 1849. He says, in concluding his report:—

> "I may state my conviction that a patient (if not too old, nor suffering from severe organic disease), however urgent may be his symptoms, if they have not been of too long duration, has, if injected with saline alcoholic fluid, a very good

prospect of recovery. Often it is also his only chance. This belief is shared in by those who have seen my cases. The patients, however, require careful watching, and if the pulse is going, immediate reinjection again and again is necessary. When a patient has been long pulseless, clots form in the heart, and, as I have seen, in two cases where injection was attempted, extend into the larger veins. In one case the fluid would not flow in, and only distended the veins of the arm injected. After death clots were found extending from the heart into the axillary vein. I have no doubt that in some of my earlier cases clots existed in the heart at the time of the injection. During the last three weeks many cases, not nearly so severe as those injected, have died; they have generally got over the collapse, but succumbed to the reactionary fever. Had those been injected, in all probability the event would have been different. The bad results, with this treatment, are due to the fact that it is often only employed when the patients are in *articulo mortis*, under the impression that the injection is dangerous. This it is certainly not."

This may be speaking too strongly, but it is our own conviction, that a measure which will restore the pulse when it is entirely gone, diffuse warmth throughout the cold body, disperse the livid hue, replace the whispering utterance with almost the voice of health, remove the cadaverous expression of the countenance, and relieve the cramps, the vomiting, and the thirst, is of considerable value, and should not be cast aside simply because it does not succeed in the majority of cases in which it is used. And this conviction is strengthened by reports of cases where the injection was used successively several times with the same results, the patient recovering apparently in consequence of this repetition of the remedy.

We have not noticed all the articles in this volume, but have selected those which contain points that would most interest the readers of this Journal. We thank the Medical and Surgical Staff of the London Hospital for this contribution to our science, and shall hereafter look for their annual volume with great interest. W. H.

ART. XV.—*The Transactions of the American Medical Association. Instituted 1847. Vol. XVII. 8vo. pp. 713. Philadelphia, 1866.*

THE *Transactions* before us, like those of former years, commence with the address of the presiding officer, who, at the present session, was Dr. D. Humphreys Storer, of Boston, Mass. Its theme is the practice of Specialities in Medicine. The question is one of great importance, and which, taken in all its bearings, is one not very easy of solution. Dr. S. treats it with the utmost candour and fairness. The question may be considered under two points of view. The first embraces the propriety of a course of medical education directed solely to the devotion of the pupil, subsequently, to the practice of some speciality—to the exclusive treatment of a particular class of diseases, or the diseases of a single organ or series of associated organs. Such an apology for medical education in the proper acceptation of the term should be unhesitatingly condemned. So closely are the pathological conditions of every portion of the organism related to each other—so powerfully does the abnormal state of one organ or set of organs influence the functions of many or of all the others, that, without an acquaintance with the general pathology and therapeutics of the entire living system, it is impossible to form a correct estimate of the special pathology and therapeutical management of the diseases of particular organs.

To the second point of view under which the question of speciality in medicine presents itself, there can be much less objection. We mean the devotion of time and talents, after a sufficiently prolonged attendance upon a liberal course of medical instruction, and graduating, to the especial study and treatment of such of the branches of his profession as may have for the individual a particular interest, or which from any circumstances may awaken an especial claim. To pursue such a course all have a perfect right, and there can be little doubt that when the attention of any one is directed somewhat exclusively to a single department of medical investigation, or to the careful clinical observation of a single disease, or class of diseases, a closer acquaintance will be acquired of the nature, causes, symptoms, and complications of the morbid conditions thus studied, and of the therapeutical management best adapted to relieve the suffering to which they give rise, to circumscribe their ravages, and conduct them to a favourable termination.

The dangers and abuses to which the toleration of specialities in the study and practice of medicine is liable to lead, are clearly pointed out by Dr. S. He would not, however, discountenance the specialist as such; he would not speak disparagingly of him; he would neither ridicule nor condemn him, but would cheerfully welcome as a co-laborer to our ranks every well-educated, honourable, honest physician, to whatever portion of our science he may devote his time and his talents. Every atom he can add to our stock of knowledge, every grain he can place upon the mound already raised should be appreciated and rejoiced in; and as long as his conduct shall prove him worthy of our esteem, so long may he claim and possess it.

The first of the scientific papers is one on the Relations which Electricity Sustains to the Causes of Disease. It was presented by Dr. S. Littell, of Philadelphia, and discussed at the last annual session of the Association, but was not fully prepared for publication until the meeting in 1866. Dr. L. assumes that it is to the morbific influence upon the living organism of certain electric conditions of the atmosphere in circumscribed districts or neighbourhoods, or throughout more or less widely-extended regions, that we are to look for the principal cause, if not the only one, of diseases of an endemic or epidemic character. The question, which is certainly one of the deepest interest and importance, particularly in our study of the etiology of very extensive classes of diseases, is very ingeniously argued by Dr. L. The correctness of the position he has assumed is not, however, satisfactorily demonstrated.

The theory advocated by Dr. L. is by no means a novel one. Electrical fluctuations have been assigned by not a few, and with more or less confidence, as the cause of certain maladies. Few attempts, however, have hitherto been made to show by argument and facts the ground upon which such assignment was made, nor has the electrical cause of disease been so fully developed, nor has it received so extended an application as is given to it in the report before us. As Dr. L. admits, "Much patient investigation, however, and careful induction will be required to remove all doubt, and secure its adoption by the profession."

The report of the Committee on Disinfectants (Drs. E. M. Hunt, A. N. Bell, and J. C. Smith) follows next. Of the main subject it discusses, and the several important questions therein involved, the report presents but a very general outline. The views presented by the Committee are in the main correct, but these are presented too much in the form of axioms to be received without argument. We are prepared to admit that "where well-regulated heat and perfect ventilation can be secured, we are in possession of all needed disinfecting powers under usual, or, we should say, under all circumstances, especially if perfect cleanliness of room, bedding, clothing, and person be added, excepting perhaps where a number of diseased persons are crowded together. In the latter case, the only disinfectant to be depended on is the separation of the sick, and placing them in a pure, dry atmosphere of a proper temperature."

The Committee present a list, with a brief commentary, of the substances which they regard as worthy of a notice, more or less extended, as their tested merits or assumed value may demand. Many of these substances are adapted to furnish, doubtless, important aid as auxiliaries in the work of disinfection; others of them will be found, we suspect, to be rather deodorants than disinfectants, in the proper acceptation of the term. We are not prepared to admit what the Committee would seem desirous to inculcate, that to remove a disagreeable or disgusting odour is, in all cases, equivalent to destroying the agent or source of infection.

In a very short paper by Dr. B. F. Craig, the use of the Permanganate of Potassa is recommended "for the purification of water, especially during the prevalence of epidemic cholera."

An Apparatus for the Treatment of Fractured Patella is described by Dr. J. M. Boisnot. The principle involved in the use of this apparatus is not new, being that of holding the parts in juxtaposition, by drawing upon the upper fragment in such manner as to bring it down, while the lower is held steadily. The form of the splint is that of a double inclined plane. Such a splint, keeping the leg *slightly* flexed, causes the patella to remain above the anterior depression between the condyles of the femur. The broad concave surface of the upper portion of the splint forms a comfortable support for the thigh posteriorly, while it serves as a point for counter exertion. The same may be said of the lower portion of the splint in its relation to the leg, with the additional recommendation of its not pressing upon the heel or tendo Achillis. The upper fragment of the patella is drawn down by pieces of leather, with attachments of common cord, which pass over pulleys to rollers notched, and provided with a spring, so that they may not slip in winding. The splints are confined to the limb at each end, by a slip of ordinary three-inch bandage. The description of the instrument is deficient in clearness from the want of any pictorial illustration.

A report is presented by Dr. J. J. Woodward, on The Causes and Pathology of Pyæmia (Septæmia). All that Dr. W. has attempted in this report is to bring together a few considerations in connection with the literature of pyæma, to offer a brief summary of the leading explanations of the phenomena which have been propounded, and a criticism of the basis on which they rest, with a few hints on further explanation. The report concludes with a sketch of some of the more prominent forms of non-surgical pyæmia, especially uterine pyæmia, that of new-born infants, that originating in intestinal ulcers and other lesions of the portal system, and the so-called idiopathic or spontaneous pyæmia.

We regret that Dr. W. had not been able to command the time requisite to present a complete monograph on pyæmia, instead of being forced to limit himself simply to a sketch of what others have done with a view to determine its true pathology. From the hints thrown out in discussing its literature it is evident to us that he is prepared to advocate a pathology of pyæmia far in advance of that entertained by a majority of his contemporaries.

The next communication is on The Clamp Shield, an instrument designed to lessen certain surgical dangers, more particularly those of extirpation of the uterus by abdominal section, by Dr. H. R. Storer, of Boston. This instrument consists of two jaws like the cutting blades of a pair of large shears, with a serrated edge where they come in contact, of the breadth of half an inch at their centre, and gradually tapering at their outer edge towards both the ends. The upper end of one blade is elongated into an odontoid process, which passes through a corresponding socket in the other; the upper end of this process is curved outward, so that when the jaws are closed it tends to hold them in position. To the lower extremity of each jaw is attached, by a ball and socket joint, the blade of a pair of narrow forceps. The blades of the forceps are united by a clasp, the pivot rotating upon its axis so as to allow closure at any angle. To the lower end of the right blade of the forceps is hinged a rod with a screw, which passes through an opening in the opposite blade. A nut on the outside of this blade, working on the screw-like rod, is capable of exerting whatever amount of pressure may be deemed advisable.

Dr. S. sums up the grounds upon which the clamp shield is to facilitate the operation for removal of the uterus and to lessen its mortality as follows:—

"I. *As a Clamp.*—It lifts the uterus, so as to bring as much as possible of the cervix within its grasp. 2. It enucleates the portion to be divided from the surrounding tissues. 3. It aids and increases the action of the ecraseur, indirectly controlling or preventing hemorrhage, and thereby, also, 4. It lessens the number of ligatures necessary to be subsequently applied.

"II. *As a Shield.*—5. It protects the tissues beneath and around it from being lacerated. 6. It prevents outlying vessels from being unnecessarily divided. 7. It covers the bladder, the vagina, and the rectum, and so avoids the causation of increased shock, and in case this is recovered from, of increased subsequent danger.

"III. *In its twofold Capacity.*—8. It relieves the surgeon's mind during the operation from all fear of the dangers above indicated. 9. It enables him cheerfully and hopefully to undertake many doubtful or desperate cases from which he would otherwise shrink in dismay, or which he could not, in conscience, undertake; and, best of all, 10. It, for all these reasons, tends to increase the number of cases attempted to be cured, and to lessen the mortality of those in whom the cure may become realized."

The next paper is on The Intra-oral Method of Operations on the Lower Jaw, by Dr. Dewitt C. Enos, of New York. A short account is first given of the mode of operating within the mouth, then a brief history of the employment and success of this plan of operating; the paper closing with a view of the advantages by which it is recommended. These are the avoidance of an unsightly and perhaps irritable cicatrix. The better preservation of the normal shape of the jaw, not only causing less deformity, but rendering its usefulness in mastication greater, since its usual coaptation with the superior maxilla is maintained. The less liability to sudden asphyxia by the retraction and swallowing of the tongue during the operation, and by removing, also, the more formidable and chronic form of asphyxia alluded to by Bégin; the non-division of the facial vessels and nerves, consequently the less hemorrhage and the better preservation of facial innervation.

Next follows a report by George M. McGill, Assist. Surg. U. S. A., advocating the adoption of A Periosteum Flap in all amputations in the continuity of a limb.

Since January, 1863, Assistant Surgeon McGill has adopted this flap in all his amputations in the continuity of bones. A flap of periosteum is outlined, raised, and adapted to the newly-cut bone surface. This flap intended to cover and unite with the newly-divided bone surface, as skin and superficial fascia are intended, in certain modes of amputation, to cover and unite with soft tissues; with this difference, however, that in a periosteal flap we have a homologous structure, to unite with the newly-cut surface of bone; while in the case of skin and fascia covering soft tissues, union is demanded of heterologous forms. Granting that *like* more readily unites with *like*, then where a periosteum flap is adopted and the newly-cut surface of the bone has not been killed by the too coarse teeth or the too rapid action of the saw, we are more likely to have union by the first intention. This *a priori* conclusion is sustained by the series of cases adduced by Assistant Surgeon McGill; for the details of which we must refer to the paper itself.

An interesting and instructive case of Paralysis of the Vocal Chords with Aphonia is related by Dr. J. SOLIS COHEN, of Philadelphia. The paralysis in this case was overcome by direct stimulation in a single operation, and the voice was restored at the same sitting by vocal gymnastics.

The Report on Vesical Calculi, by Dr. J. C. Hughes, of Iowa, is based upon a case in which lithotomy by the bilateral method was performed successfully four times, within a little more than five years in a patient aged, at the time of the first operation, 62 years. The patient is still active, in his 74th year, and enjoying comparatively good health. At the first operation two calculi were removed of the triple phosphate variety; at the second, one calculus; at the third, four calculi, one of which was located in the fundus of the bladder and encysted; at the fourth operation twelve calculi were extracted. Of the entire nineteen calculi accurate lithographic drawings are given, showing their respective shapes and sizes.

Dr. L. A. Sayre, of New York, relates a case of Luxation of the Femur into the Ischiatic Notch, of Nine Months' Standing, Reduced by Manipulation.

Dr. Alfred C. Post, of New York, describes an Instrument for Performing Lithotomy by a Bilateral Section of the Prostate, devised by him to secure the advantages of the gorget, and, at the same time, to guard against its inconveniences and dangers.

The instrument consists of two parts. 1st. A straight steel rod eleven inches in length; the distal half of which is cylindrical, and of the size of a No. 1 bougie, terminating in a rounded button of the size of a No. 5 bougie. The proximal half of the rod is a rectangular four-sided bar, whose diagonal corresponds with the diameter of a No. 5 bougie. The proximal end of the rod is furnished with a flat ivory handle, roughened on its sides to enable it to be held more securely. 2d. A silver canula of the size of a No. 10 bougie, and occupying one-half the length of the rod. The distal extremity of the canula is tapered off to the extent of three-quarters of an inch, so as to form a beak, which projects beyond the blades. Immediately behind this beak are two blades which are inserted into grooves on the sides of the canula, so as to be readily taken out and replaced. There are four sets of blades. The largest makes an incision into the neck of the bladder of about one inch and a half in width, and the smallest about three-quarters of an inch. The proximal extremity of the canula has a small ivory handle adapted to it, by means of which the canula is made to stick upon the rod. The end of the canula next to the handle is provided with a square mortice hole, by which it moves upon the rectangular bar.

The manner of operating with this instrument is as follows. The patient being placed and secured in the usual position, a grooved staff is passed through the urethra into the bladder and held firmly in its position by an assistant. A crescentic incision is then made through the perineum with its concavity towards the anus. The incision crosses the raphé of the perineum about an inch in front of the anus, its horns extending each side, midway between the anus and the tuberosities of the ischia. A free incision is now made into the membranous part of the urethra along the groove of the staff. The button at the extremity of the steel rod is then passed into the bladder, and the staff withdrawn. The surgeon then holds the handle of the rod with the thumb and index finger of the left hand, and the handle of the canula with the corresponding thumb and finger of the right hand. The canula is then pushed forward until the blades with which it is armed have penetrated the prostate, and the sense of the resistance has ceased, indicating that the blades have entered the cavity of the bladder; the canula is now retracted and then the whole instrument is withdrawn. Nothing remains but to introduce the forceps, seize and extract the stone.

"More than twenty years have elapsed," Dr. P. remarks, "since I introduced this instrument, and I have had occasion to employ it in a considerable number of cases. Experience has confirmed my opinion of its usefulness, and I have no hesitation in commending it to the profession as a means of diminishing the dangers of a formidable operation."

By the same gentleman a description is given of an original Instrument to Facilitate the Introduction of Insect Pins in Wounds of the Face and of other parts, where it is important to secure an accurate adaptation of the edges of the wound. The instrument consists of a needle two and a half inches long; its diameter corresponding with that of a very large darning needle, slightly curved to the extent of a quarter of an inch from its point, and having at its proximal extremity a small flat handle. The curved portion of the needle is flattened, and has cutting edges. Throughout the greater part of its length on the side corresponding with the convexity of the curve there is a groove. In using the instrument, the needle is passed through the edges of the wound in the position to be occupied by the pin, which latter is then slipped along the groove on the needle. By this means the most slender pin may be passed through the skin with perfect facility and without being bent out of shape. The needle being withdrawn the pin is secured by a thread wound upon it in the form of a figure eight, and its point is then removed by means of a cutting pliers.

The next report is from the Committee on Diphtheria as it has Occurred n the United States, by Dr. H. D. Holton, of Vermont. This report presents a very good history, though by no means a complete one, of the prevalence of diphtheria in different parts of this country. The remarks of its author on the pathology of the disease and his description of its most usual symptoms, march, and sequelæ, and his directions for its treatment, are in the main correct. We should differ from him, it is true, as to he contagious nature of diphtheria, and upon some other points of less nportance. We would suggest to Dr. H. that there is too close an ffinity between denuded cutaneous tissue and mucous tissue, to render the ability of the latter to diphtheritic deposition any argument to prove that hose have fallen into error who believe that diphtheria is "a peculiar nflammation of the mucous membrane," into which always the membraniorm exudation takes place.

The conclusions to which the reporter has arrived are thus summed up:—

"1. That diphtheria has occurred as an epidemic, from time to time, from the first settlement of this country. 2. That it is a distinct disease, and is not to be confounded with scarlatina or croup. 3. That it is particularly a disease of childhood, although it exempts no age. 4. That it is communicable to that degree that it is the duty of every physician to separate the infected from such as are well, particularly children. 5. That it is a disease primarily affecting the blood, consequently the treatment, to be effective, must be not local but general, and of such a nature as to eradicate or neutralize the poison. Such local treatment should, however, be used as will prevent the absorption of the fluid portions of the exudation. 6. That when the exudation has invaded the trachea, the only hope of saving the patient is in tracheotomy."

The report which follows On Spotted Fever, so called, by Dr. J. J. Levick, of Philadelphia, is one of the ablest and most complete of any comprised in the present volume of *Transactions*. The views of the author of the report in respect to the pathology of the disease and its relationship to cerebro-spinal meningitis seem to us to be borne out by all the facts that have been reported by different observers, from an early period in the present century, and of various and different localities. We prefer not to enter upon an analysis of Dr. L.'s report on the present occasion, as in an early number of the Journal we expect that this subject will be more fully discussed than it is possible for us to do on this occasion.

A paper is presented by Dr. A. Mason, of Massachusetts, on The Use of Aluminum as a Base for Artificial Teeth, as a Substitute for Gold and other Metals, and Red Vulcanite. It is a valuable contribution to mechanical dentistry.

A very sensible and suggestive report on The Etiological and Pathological Relations of Epidemic Erysipelas, Spotted Fever, and Diphtheria, was presented by Dr. N. S. Davis, of Chicago. When examined by a more extended series of observations than we at present possess, we have no doubt that the views shadowed forth in the present report will be found correct, and that the inquiry carefully carried out in respect to other diseases than those enumerated, a close relationship in the nature and causation of a more extended list of epidemic maladies will be found to exist than is now suspected.

A report was presented by Dr. H. R. Storer, of Boston, a delegate from the American Medical Association to the Association of Superintendents of Asylums for the Insane, at its session of 1865-66, and especially instructed to urge upon the latter body "the advantage of a more intimate union with the former, alike for the purposes of science and effectual practice." The delegate failed in the accomplishment of the main object of his mission. The report closes by urging the adoption by the Medical Association of the following resolutions:—

"1. That the Association recommend to the several medical and law schools of the country the establishment of an independent chair of medical jurisprudence, to be filled, if possible, by teachers who have studied both law and medicine. Attendance upon one full course of lectures from whom shall be deemed necessary before the medical degree is conferred.

"2. That while this Association regrets that the Association of Superintendents of American Asylums for the Insane has not yet thought fit to unite itself more closely with the representative body of American physicians, it still is of opinion that such union is for their mutual and reciprocal advantage, and that it ought to be effected without delay."

Dr. B. Haskell presents a comparatively short paper in continuance of the subject discussed by him in a communication read at the session of the

Association for 1865, in respect to the existence of independent sensitive and motor nerves; a doctrine which he has attempted to show is not an absolute truth. To learn fully the character of Dr. H.'s objections to the views of Bell, and the arguments upon which they are based, his entire paper, in connection with the communication formerly presented by him, must be read *in extenso.*

Health in Country and Cities, is the title of a communication presented by Dr. W. F. Thoms, of New York. It is illustrated by tables showing the death-rates, sickness-rates, etc. etc., in country and cities. The communication of Dr. T. is a very valuable contribution to the department of hygeine The question of the comparative health of cities and country is a very complex one, requiring for its settlement not only a long series of statistics, but a careful comparison of these statistics, with a long list of questions concerning the sanitary condition of different cities and the different parts of a city, and of the several tracts of country and rural districts from which the statistics are derived, with the extent, condition, character, and pursuits of the inhabitants of each.

The report of the Committee on Medical Literature was presented by Dr. Charles A. Lee, of New York. It is a well written and able paper, presenting a very fair exposition of the present condition and extent of the medical literature of the United States, and of the character and value of our medical periodicals. While he vindicates the general medical literature of our country in the face of the unjust criticisms to which it has been subjected, even in many of the annual reports of the Association; and admits whatever deficiencies it may be justly chargeable with, he still insists that whether we examine the medical literature of this country in reference to value of material or to excellence of style, to all, indeed, that goes to constitute true scientific authorship, our best American writers are about on a par with those who have treated similar subjects in Europe.

> "We think," says the reporter, "we may safely say that the time is come when our medical students, as well as practitioners, may rely chiefly on our own writers for the professional instruction and information they need; and it is but an act of simple justice to our native authors to urge their writings upon the attention of physicians, as far better adapted for their wants, and far safer guides in the management of our diseases than any foreign works whatever."

The next report, from the Committee of Medical Ethics, is On Specialities, by Dr. Worthington Hooker, and is a very well written and sensible paper. The reporter takes ground in opposition to the exclusive study and practice of specialities, and his arguments have great force and are well worthy of consideration. Appended to it is a very brief minority report, by Dr. Henry I. Bowditch, in defence of such study and practice.

A very candid report was made by Dr. Charles S. Tripler, from the Committee on Rank in the Medical Department of the Army. The comfort and efficiency of the medical officers of the army and the good of the service generally, call loudly for the recognition of military rank in the members of the medical department of the army; and it is to be hoped that the subject which is now before our National Legislature will be disposed of without delay and in accordance with the wishes of the members of a portion of the military service, second in importance to neither of the others.

A communication on the subject of Patent Rights among Medical Men was presented by Drs. D. Prince and Thomas Antisell, in which is advocated the propriety of medical men securing to themselves by a patent, a profit from the use of any discovery or invention of theirs, whether a medicine or a surgical instrument or apparatus, and proposes to rescind the rule in our code of ethics in respect to patents, which rule is declared to be "founded upon misconception of the subject of patent laws and rights." The reasoning in the report referred to is specious rather than sound, and would equally sustain the right of the medical man to deal in secret nostrums and to withhold from the profession generally the results of his own experience and observations, however important these may be as aids in the prophylaxis or management of disease.

Dr. Benjamin Lee, of Philadelphia, contributes a paper on The Correct Principles of Treatment for Angular Curvature of the Spine, in which is advocated the adoption of means by which the weight of the head may be so far removed from off the spinal column, that even commencing deformity may be arrested and future angulation of the spine guarded against, whilst sufficient time is allowed to conquer if it be possible, and in many it is, any disease of the vertebræ, from which angular curvature may result. We do not subscribe to the employment of the mechanical means suggested by Dr. L. for the prevention and correction of spinal curvature, indiscriminately in all cases of vertebral disease; we admit, however, the general correctness of the principles of treatment which he has laid down.

The volume closes with an Essay on Digitalin, its Chemical, Physiological, and Therapeutic Action, by Dr. Samuel R. Percy, of New York. To this essay was awarded the prize of the Association. The essay is a very elaborate one, and gives a very good account of digitalin—a neutral principle obtained from the leaves and seeds of digitalis; its chemical history—the process for obtaining it, its physical properties, and chemical tests and reagents; together with an account of its physiological action on inferior animals and on man; its therapeutic value and modus operandi.

Digitalin, in its medicinal action, controls and lessens the frequency of the heart's pulsations; in fuller doses it deranges the actions of the heart; in poisonous doses it paralyzes the heart, and causes death by syncope.

> "It is a pure sedative, and has no narcotic action. Its depressing action is preceded by no stimulation, and the effects it produces upon the mind when given in large doses, causing dreams and hallucinations, is but a reflex action, a derangement of nervous influence."

In comparing the action of digitalin with other sedatives, we find that, unlike hydrocyanic acid, it does not paralyze the medulla oblongata and leave the heart beating after the brain ceases to act. "It produces no anæsthetic effect upon the nerves of the stomach, like creasote." It does not, like aconite, produce an increased secretion of saliva, or paralysis of the fauces, or anæsthesia of the peripheral nerves. Unlike conium, it produces no paralysis of the motor nerves; nor does it cause, like colchicum, purgation, nor congestion of the kidneys, nor increased secretion of uric acid. Unlike antimony, it does not diminish the amount of fibrin in the blood, and become in large doses tolerant, nor, like ipecacuanha, does it produce any great increased secretion from the stomach and lungs.

> "I have not," remarks Dr. P., "been able to verify the diuretic action of digitalin when given in a state of health; when given where cardiac lesion and dropsical effusion are present, it is, as a whole, the best diuretic we possess. The nausea and vomiting that are sometimes seen when digitalin is freely given, is nothing but the derangement of the function of the vagus nerve. I have not seen that it acts as an irritant to the mucous membrane of the stomach or intestines, though Bouchardat mentions this pathological condition, but it must be remembered that he tried the œsophagus." D. F. C.